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NOTES AND LITERATURE

HEREDITY

One of the most important papers relating to heredity that has appeared in recent months is that of Tower, dealing with hybridization investigations with species of the genus *Leptinotarsa.*[15] I shall not here attempt an extensive review of this paper, but mention it rather as a means of calling attention to its importance and suggesting that any one interested in theoretical discussions of heredity should not fail to read it. Tower has done an immense amount of work with this genus. His results lead him to accept the factorial hypothesis as an explanation of Mendelian phenomena but to discard wholly the de Vriesian interpretation of these factors. The most important contribution in this paper is the apparent fact that the environment at the time when eggs are fertilized may change very materially the nature of the hereditary factors. It is unfortunate that the author does not give more details in connection with this conclusion. The data he does give are mixed and contradictory. There has possibly been an error in printing Tower's paper, but if not there was a serious error in its preparation, as will be seen from the following. In experiment 409, which was several times repeated, the results were exactly as if one of the parents had been a heterozygote between the two species. F_1 consisted of two types, one of which was identical with the female parent and the other intermediate between the two. The one like the female parent bred true to that type, while the other behaved in all respects as a heterozygote. In experiment 410 the same two species were utilized, but the temperature and humidity conditions at the time the eggs were fertilized were made quite different. This experiment, which was repeated eleven times, gave in every case ordinary Mendelian phenomena.

In experiments 409/411, which was performed seven times, one set of eggs from the same cross as above was produced under conditions identical with those of experiment 409, and in each case gave identical results with those of 409; that is, F_1 consisted of two types, one heterozygote, and the other homozygote of the maternal type. Using the same individuals which produced a set of eggs of this kind to secure another set of eggs produced under the conditions of experiment 410, the results in each of the seven experiments gave F, which behaved in all respects as a homozygote of the maternal type. This fact is set forth in considerable detail and Plate III illustrates it just as here described. This result occurred in all cases whether the set of eggs produced under the conditions of experiment 410 was produced before or after the set which gave the results of experiment 409. Now the remarkable thing about this experiment is this. While in experiment 410 the results in each of the eleven cases gave ordinary Mendelian heterozygotes in F_1, in each of the seven cases of 409/411 the eggs produced under the same conditions as 410 gave homozygotes of the maternal type. Thus, the conditions of experiment 410 in eleven cases gave one result, in seven other cases they gave an entirely different result, and the only difference in the conditions was that in 409/411 the female either had produced or was in the future to produce a set of eggs under the conditions of experiment 409 (p. 295).

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[15] Tower, Wm. L., "The Determination of Dominance and the Modification of Behavior in Alternative (Mendelian) Inheritance, by Conditions Surrounding or Incident upon the Germ Cells at Fertilization," *Biol. Bull.*, XVIII, No. 6, May, 1910.

Thus, on page 294, in describing experiment 410, it is stated that experiment 410 gave F_1 all heterozygote; on page 330 it is stated that experiment 410 gave F_1 all homozygote of the maternal type; on page 295–6, in describing experiment 409/411, it is stated that, under the conditions of experiment 410, this experiment gave F_1 all homozygotes of the maternal type; and on page 304 it is stated that experiment 409/411, when performed under the conditions of 410, gave results identical with those of 410. These statements are directly contradictory. We must withhold judgment on this point of influencing the hereditary factors at the time of fertilizations, until Mr. Tower informs us which of these statements are correct.

In these exceptional cases, where the F_1 hybrid behaved as a homozygote of one of the paternal races, the author does not tell us whether the F_2 and later generations were each time produced under the conditions which produced the aberrant F_1. One would infer, however, that they were not, and that the change which occurred in the fertilization of the eggs which produced F_1 was permanent and not reversible. It is hoped that he will give us fuller data on this point in future papers which are promised.

In several cases Tower mixed three species which interbred freely and left them under natural conditions for several years. A careful study of the progeny in each case showed that a new type arose, consisting of a complex of the characters of the old types, and that this new type rather rapidly replaced every other type, although some of these other types were known to be quite capable of existing under the conditions of the experiment. This would indicate that in some way the new type had a distinct advantage over the other types with which it competed for food, or possibly the repeated crossing of the types was in some way inimical to all the types except the one. Experiments of this character show that hybridization may be an important factor in the development of new varieties or possibly new species.

From the fact that when the same species are mixed together in two places where the conditions are different, the resulting type which finally wins out and becomes practically the sole representative of the mixture, is different under different conditions. Tower draws the conclusion that the conditions surrounding the germ cells at the time of fertilization "profoundly modify the behavior and the relationships of the characters entering into the crosses." This conclusion seems hardly justified. Of course it may be correct, but the well known fact that from complex crosses of this kind a great many types may result from what we know of the behavior of Mendelian characters and that these types would naturally bear different relations to the environmental conditions offers apparently a much simpler explanation of the reason for the survival of the one type under one set of conditions and another type under another set of conditions. It seems hardly necessary to assume that the conditions existing at the time of the fertilization of the egg determine the characters which were to result from the fertilization to explain this particular phenomenon.

Subsequent investigation of these new types of mixed origin showed that in all cases they occasionally produced individuals different from the general population but which in all cases exhibited characters which were present in the original parents of the complex mixture. Tower repeatedly compares this phenomenon to the phenomena which de Vries observed in *Œno-*

thera lamarckiana, and suggests that the mutations which de Vries observed are probably due to previous hybridization. This is a very interesting suggestion, but the writer is inclined to believe that the phenomena observed by de Vries were due to a different cause. It is definitely proved that in some of de Vries's mutants the chromosome numbers are different from those of the parent form. Cytological investigations have also shown that in the reduction division in these *Œnothera* mutants there are frequent irregularities in the distribution of chromosomes. It seems probable that de Vries's mutations are not the result of previous hybridizations but rather are due to irregular behavior of chromosomes in the reduction division. If this is true then the phenomena observed by de Vries would be due to a different cause from that which presumably produced the results which Tower observed. In the case of Tower's results we can explain the facts by the assumption of simple Mendelian segregation. In de Vries's work there is evidence that the phenomena are due to a different cause.

It is gratifying that Tower takes a very broad view of the factorial hypothesis of Mendelian phenomena. On page 323 he remarks:

> This factorial point of view is in no wise, of necessity, to be tied to or confounded with such speculations as the id-determinant-biophore fabric of Weismann, nor with the pangene complex of de Vries, which have no foundation in fact.

This is the view which the writer has held for years and has frequently set forth in these pages. I have also frequently pointed out that we do not yet have sufficient knowledge of the physiological processes of living matter to permit us at the present time to formulate an adequate theory of the phenomena observed in hybrids. I think we can, however, point out the general nature of the causes underlying these phenomena, as I have attempted to do in my theory of Mendelian phenomena.[16] In speaking of the difference in germ cells with respect to given characters, he has the following to say:

> What this difference in the gametes is we do not know, but observed behaviors are interpreted as being, most probably, due to the mechanical separation into different germ-cells of whatever it is that produces the contrasting attributes—segregation during gametogenesis.

[16] *American Breeders' Magazine*, Vol. I, No. 2.

He further remarks on page 328:

> At present in biology we have no business with ultimate conceptions, and the two thus far attempted of germinal composition—the "particulate conception" and the "crystalline entity" are both equally dismal failures and equally useless as working hypotheses.

The statement on page 335, that characters which Mendelize are in the main unimportant attributes of the organism and only rarely are of importance in the struggle for existence, is a little bit strong. Apparently it would have been better to state that those characters which have been shown to Mendelize are of this nature. Unfortunately, most of the work of the Mendelians has been done with these superficial, easily observed characters. I see no reason why any character whatever might not, from the failure of some chromosome to perform a usual function, give a variation which would behave in Mendelian fashion if the resulting type were capable of propagating and crossing with the parent type.

Tower's paper will undoubtedly have an important influence on biological thought, as it deserves to have.

W. J. SPILLMAN.